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SPECIFIC GENESIS.

[The following communication receives insertion, contrary to the ordinary rule of the North American Review, owing to the interest attached to the subject discussed. All further comment will be reserved for a subsequent number.]

THE EDITOR OF THE NORTH AMERICAN REVIEW:—

SIR, — The rapid growth of physical science, and the constant publication of ever-new observations, make such demands on the time of naturalists that an author actively engaged upon a subject covering the whole field of biology cannot be expected to reply directly to critics, unless under very exceptional circumstances.

I have to thank Mr. Chauncey Wright for having been so obliging as to devote much space, and necessarily a considerable portion of his valuable time, to an examination of my recent work, the “Genesis of Species.” Nevertheless I must confess that, with all respect for his conspicuous talents and for his deserved reputation, I should not have undertaken the following few words of explanation but for his paper’s wide circulation in England and elsewhere by Mr. Darwin.

Any criticism published by Mr. Darwin himself, or by Professor Huxley, I should always deem it a duty respectfully to consider and, if possible, reply to; and the very extensive circulation by Mr. Darwin of a reprint of Mr. Chauncey Wright’s remarks, appears to me to amount to such an implied adoption of them, as to demand for them a consideration somewhat similar to that which I should accord them were Mr. Darwin himself their author.

Mr. Wright’s criticism touches upon so many matters of detail that it is not altogether easy to ascertain his main objects. Having, however, considered his remarks with that care which my esteem for his opinions makes incumbent on me, I venture to express my belief that, neglecting minor matters, his criticism is mainly directed to the assertion of two points.

One of these is, that I have misrepresented Mr. Darwin’s views, and have been guilty of involuntary injustice with respect to the natural forces which, according to our great naturalist, have determined specific forms.

The other is, that I have attributed an irreligious tendency to Mr. Darwin’s writings which they do not, in fact, possess; and that this is in part owing to my defective knowledge, in part to early prejudices.

Thus Mr. Wright speaks of my “theological education” and my “schooling against Democritus.” It is a matter of wonder to me who could have so misled Mr. Wright. Though reluctant, in the extreme,

to obtrude such private and personal matters before the public, I must nevertheless, in justice, observe, that my schooling has been of the very opposite character, and perfectly in unison with that which Mr. Darwin himself would favor. Only at length, and with difficulty, have I struggled out of that philosophy of "nescience," the evils and the fallacies of which are so apparent to me because, at one time, its doctrines so completely possessed my assent.

With regard to Mr. Darwin's theory of the origin of species, I should hasten eagerly to acknowledge my error if I had been guilty of injustice with respect to it, and also to thank any critic who had been so kind as to call my attention to such unintentional unfairness. I must confess, however, that I cannot detect that misrepresentation in my "Genesis of Species" which Mr. Wright seems to there discover.

In common with so many others I was, at one time, a hearty and thoroughgoing disciple of Mr. Darwin, and I accepted from him the view that Natural Selection was "*the* origin of species." It was only by degrees, and through the evidence of a multitude of biological facts, that an opposite conclusion was gradually forced upon me. Having come to that conclusion, on scientific grounds only, after careful reconsideration of those grounds and much discussion of the subject, I ventured to publish my "Genesis of Species." Therein I endeavored to bring before the public the leading facts which had produced the conviction in my own mind that Natural Selection was *not* the origin of species, *not* the main determining agent in the fixation of specific characters; although I allowed that it played, and necessarily must play a certain subordinate part.

This conviction had forced itself on many minds before the publication of my book, and since then has approved itself to the minds of many more. Indeed, Mr. Darwin himself seems to have come round substantially, though not avowedly, to the same opinion, and has, in his "Descent of Man," implicitly admitted, though he has not yet explicitly declared, that Natural Selection is *not* the origin of species. I cannot but confess that it appears to me even Mr. Chauncey Wright himself concedes all that for which I contend, though he at the same time seems to imagine that he asserts the validity of Mr. Darwin's original position.

No one could be less disposed than I am to detract from the great merit unquestionably due to Mr. Darwin, or to ignore the vast impetus which his views have given to the wide reception of the doctrine of evolution. Nevertheless, we must not allow our just admiration for the zeal, genius, and courage of Mr. Darwin to blind our eyes to two facts. One of these is that an important part of Mr. Darwin's theory was not new,

but, on the contrary, very old. The other is, that though the *popular* acceptance of evolution has been brought about through him, yet that the minds of *scientific* men were well prepared for, and disposed towards, evolution years before the appearance of "The Origin of Species."

Biological facts, by their gradual accumulation, had long been predisposing scientific minds to the acceptance of this theory. I myself, indeed, fully accepted it, and I found that a similar acceptance existed in the minds of others, notably in that of Professor Owen. Mr. Wright, therefore, is certainly correct, in this sense, when he says that "it is not to what is now known as 'Darwinism' that the prevalence of the doctrine of evolution is to be attributed or indirectly assigned." The part of Mr. Darwin's theory which is old is that which attributes so much importance to the destructive powers of nature, a view advocated by Lucretius and treated of by Aristotle in the passage quoted in my book.

What, however, was unquestionably Mr. Darwin's own, was the remarkable conception that this exterminating power, acting upon organisms presenting slight variations, so overbore all other influences as to occasion the survival of the fittest variations, and in this way (by a process of cutting off and limiting) fixed the characters of the different organic species, thus becoming their origin. *The* origin, not, of course, of the slight variations, but of the fixing of these in definite lines and grooves.

Gradually, however, the arguments of opponents have forced upon Mr. Darwin's active and candid mind modifications of his views, till, as I have said, he has come to admit in principle that Natural Selection is not *the* origin of species. I cannot myself see that there is, in this change of view, anything at all derogatory to Mr. Darwin; and for my part, my esteem for that illustrious naturalist is strengthened rather than weakened when I read candid admissions of antecedent error. These admissions should not be brought forward, save when an unscientific appeal is made to his *authority*, or when an advocate more zealous than judicious attempts to deny that Mr. Darwin's opinions have undergone any grave modifications. Then indeed truth and justice demand the production of such admissions. They do so since the assignment of the law of Natural Selection to a *subordinate* place is *manifestly* an abandonment of the Darwinian theory as originally proposed; for how can that be said to be *the origin* of species which only co-operates, in an inferior and comparatively unimportant manner, in determining that origin?

Mr. Chauncey Wright's remarks seem to me, then, to render necessary a reference to these earlier statements of Mr. Darwin. A num-

ber of such statements* and admissions of our great naturalist — not, indeed, his earliest, but from the *third* edition of “The Origin of Species” — were recently brought forward in the July number of the “Quarterly Review.” They appear to have been published for the purpose of guarding the public from a hasty acceptance of Mr. Darwin’s dogmatic expressions, merely in deference to his *authority*, and without a careful estimate of the value of the facts brought forward by him.

The passages referred to seem to me to contain statements amply sufficient to repel Mr. Wright’s charge against me of injustice to Mr. Darwin, and to show, on the one hand, that the original theory of the origin of species was such as I have represented it to have been; and, on the other, that Mr. Darwin has, in fact, abandoned the position which he originally took up.

From the passages referred to we may learn that Mr. Darwin, even so lately as in his third edition of the Origin, considered that Natural Selection acts only by numerous slight modifications of special use to the organisms possessing them. In fact, that he completely stakes the whole of his theory on the non-existence or non-action of causes of any moment other than Natural Selection; it being the essence of that theory to recognize only the conservation of slight variations directly beneficial to the creature which possesses them, by affording it better means either of obtaining nourishment or of eluding or outstripping its enemies or of reproducing its kind.

From “The Descent of Man,” however, we find that Mr. Darwin now recognizes and admits that he had “probably attributed too much to the action of Natural Selection,” and that he “had not formerly sufficiently considered the existence of many structures which appear to be, as far as we can judge, neither beneficial nor injurious.” He also acknowledges that he has been too hasty in ascribing the development of certain structures, such as *Mammæ erraticæ*, to reversion, adding that “the force of the argument is greatly weakened, or, perhaps, quite destroyed.” More remarkable still we have the noteworthy confession, “I have fallen into a serious and unfortunate error, in relation to the sexual differences of animals, in attempting to explain what seemed to me a singular coincidence in the late period of life at which the necessary variations have arisen in many causes, and the late period at which sexual selection acts. The explanation given is *wholly erroneous*, as I have discovered by working out an illustration in figures.”

* These are to be found in “The Origin of Species,” 3d edition, pp. 208, 214, 220, 223; 5th edition, p. 104. “The Descent of Man,” Vol. I. pp. 125, 152, 154, 223; Vol. II. pp. 176, 198, 387, and the postscript at the beginning of the volume. “Animals and Plants under Domestication,” Vol. II. p. 57.

Mr. Darwin is most justly entitled to all honor and esteem for his candor in making these admissions ; but we must not allow such feelings to blind us to the importance of the admissions themselves.

We have, however, yet more explicit declarations as to the occurrence of characters for which not only his theory will not account, but which, in his own words, annihilate his theory. He has told us in "The Origin of Species" that this fatal consequence would ensue from the discovery of characters not produced by slight *beneficial* modifications, and yet we now read : —

"No doubt man, as well as every other animal, presents structures which, as far as we can judge with our little knowledge, are not now of any service to him, nor have been so during any former period of his existence, either in relation to his general conditions of life, or of one sex to the other. Such structures cannot be accounted for by any form of selection, or by the inherited effects of the use and disuse of parts."

Besides all this, in the fifth edition of "The Origin of Species," p. 104, we find the following significant passage : —

"Until reading an able and valuable article in the 'North British Review' (1867), I did not appreciate how rarely simple variations, whether slightly or strongly marked, could be perpetuated."

Finally, Mr. Darwin recognizes that he was formerly "inclined to lay too much stress on the principle of protection, as accounting for the less bright colors of female birds," and speaks now as if what he at one time favored in this respect was quite an unlikely matter, saying : —

"Is it probable that the head of the female chaffinch, the crimson on the breast of the female bullfinch, the green of the female chaffinch, the crest of the female golden-crested wren, have all been rendered less bright by the slow process of selection for the sake of protection? *I cannot think so.*"

I *also* cannot think so, nor can I so think with regard to those numerous instances brought forward in my book as examples of characters for the origin and development of which Natural Selection will not, I believe, account.

Every respect and deference ought to be shown to a naturalist such as Mr. Darwin, but deference has its limits and must not be exercised to the sacrifice of truth, and truth compels the recognition of the important modifications above noticed. It is not only, however, critics that dissent from Mr. Darwin's views who recognize the existence of these changes. Mr. Darwin's authorized interpreter, Professor Huxley, has lately told us the highly significant fact that Mr. Darwin is even inclined to reply in the affirmative to the question whether a variety

"can be perpetuated, or even *intensified*, when selective conditions are indifferent, or perhaps *unfavorable* to its *existence*." A more complete repudiation in principle of the origin of species by Natural Selection it would be difficult if not impossible to imagine.

Mr. Darwin has not, however, so far as I know, explicitly declared what Professor Huxley tells us he is inclined to admit. He has certainly made many important and significant admissions, but there is one more which consistency seems to demand as the logical outcome of others above cited: I mean the admission that the attribution to Natural Selection of the main determining office in the fixation of specific characters has also been "a serious error," whether it be not rather a fortunate than an "unfortunate" one.

Mr. Wright challenges the production of a sudden adaptive modification of a race, wild or domesticated, "not referable by known physiological laws to the past history of the race on the theory of evolution." In this statement I must in the first place object to the introduction of the words "on the theory of evolution," as that theory, far from being opposed, is, on the contrary, adopted and contended for by me, and I do not understand how Mr. Wright can have inserted them unless by inadvertence. Instances, however, of modifications, the production of which he desiderates, can readily be supplied. Thus the Cashmere sheep, when transferred to Europe, lost their long wool in a few generations, and this could not possibly have been due to Natural Selection. Again, the marine animals now living in Swedish lakes have become remarkably transformed, and the instance noticed by Mr. Darwin as to the Mediterranean oyster, though not evidently adaptive, is probably so, and if so would be in point. There was, however, no need to bring such cases forward, for surely it was fair to take Mr. Darwin's own estimate of what facts *he* would consider fatal, and such facts I claim to have brought forward, in sufficient number, in my book. I can only express my profound regret that I should be so unfortunate as to seem to Mr. Chauncey Wright to have made an "unfathomable translation" of the theory of Natural Selection. Mr. Darwin nowhere himself says, with Mr. Wright, that the "slightness" of the variations he speaks of "is only relative to the differences between the characters of the species"; and I cannot but think Mr. Wright himself misconceives Mr. Darwin's meaning, for I believe the latter gentleman would not speak of the sudden development of a large proboscis, like that of *Semnopithecus nasalis*, as a "slight" variation.

An admission which Mr. Darwin makes, and which I considered and consider to be important, is sought to be explained away by Mr. Chauncey Wright in a mode I cannot think admissible. He tells us

that when Mr. Darwin says that the goose "seems to have a singularly *inflexible* organization," Mr. Darwin's "*obvious* meaning" is, "that the goose *has been much less changed by domestication* than other domestic birds." Certainly if Mr. Darwin had meant this, he would not have used the word "inflexible," but "unmodified," "inflexed," or some equivalent expression. To have a "singularly inflexible organization" is to have one which *cannot* without great difficulty be modified, not one which, as a fact, *has* not been modified.

Similarly where Mr. Darwin speaks of "a whole organism having become plastic and *tending* to depart from the parental type," Mr. Wright asserts that Mr. Darwin means "capable of being moulded, or fashioned to the purpose, as clay." This is to credit Mr. Darwin with the enunciation of a truism which I am sure he would never have written. The words "tends to depart"* are plainly a repetition and explanation of the epithet "plastic," and fix its meaning. Mr. Darwin here evidently predicates an existing predisposition, and not a mere state of indifference. By "*tends to depart*" he cannot mean "capable of being made to depart," for that would not indicate any influence which has affected the "whole organization," as by his hypothesis every organism is "capable" of being modified.

I will now turn to the second matter of argument, that in which Mr. Chauncey Wright treats of the alleged possibly irreligious tendencies of Mr. Darwin's theory, and of my incompetency in physics and ignorance of the experimental philosophy.

He says:—

"Mr. Mivart has made the mistake, which nullifies nearly the whole of his criticism, of supposing that 'the theory of Natural Selection may (though it need not) be taken in such a way as to lead men to regard the present organic world as formed, so to speak, *accidentally*, beautiful and wonderful as is confessedly the haphazard result.' (p. 33.) Mr. Mivart, like many another writer, seems to forget the age of the world in which he lives and for which he writes,—the age of 'experimental philosophy,' the very stand-point of which, its fundamental assumption, is the universality of physical causation. This is so familiar to minds bred in physical studies, that they rarely imagine that they may be mistaken for disciples of Democritus, or for believers in 'the fortuitous concourse of atoms,' in the sense, at least, which theology has attached to the phrase."

I feel a little difficulty in replying to this criticism, because I cannot bring myself to attribute to Mr. Wright such a misapprehension either

* The omission of the words "in a slight degree" in my book was purely accidental. As, however, the question is one of *principle*, I do not see that the omission was of any importance.

of my meaning or of that of the school of Democritus as seems necessary to explain it.

I would willingly suppose that an obscurity of expression on my part is alone to blame, but in using the word "accidentally" I qualified it by the prefix "so to speak." But even had I not done so, I could not have imagined that any one would think me unaware that the various phenomena which we observe in nature have their respective phenomenal antecedents. It is extremely difficult to me to think that Mr. Wright can suppose I held the opinion that the phenomena of variation, etc. are not determined by definite physical antecedents. Yet, if he does not so suppose, how can he assert that when I use the expression "accidentally" I mean anything antagonistic to physical causation?

On the other hand, Mr. Wright cannot suppose that the old atheistic philosophy held events to be accidental in the strict sense, for he knows very well that Democritus and Empedocles and their school no more held phenomena to be undetermined or unpreceded by other phenomena than do their successors at the present day.

My meaning, which I rashly imagined plain enough, was that Mr. Darwin's theory might be so taken as to oppose the conception of *design* in the same way as the old Ionian theory opposed that conception. That I was fully justified in expressing such an opinion is, I conceive, plain, from the language employed by Mr. Darwin himself. In his work on Animals and Plants under Domestication, Mr. Darwin considers the building of an edifice from broken fragments of rock, and makes use even of strong expressions of the kind referred to. He says:—

"In regard to the use to which the fragments may be put, their shape may STRICTLY be said to be *accidental*. . . . If the various laws which have determined the shape of each fragment were not predetermined for the builder's sake, can it with any greater probability be maintained that He specially ordained, for the sake of the breeder, each of the innumerable variations in our domestic animals and plants. . . . But, if we give up the principle in one case,—if we do not admit that the variations of the primeval dog were intentionally guided, in order that the greyhound, for instance, that perfect image of symmetry and vigor, might be formed—no shadow of reason can be assigned for the belief that the variations, alike in nature, and the result of the same general laws, which have been the groundwork through Natural Selection of the formation of the most perfectly adapted animals in the world, MAN INCLUDED, were intentionally and specially guided. However much we may wish it, *we can hardly follow* Professor Asa Gray in his belief that 'variation has been led along certain beneficial lines,' like a stream 'along definite and useful lines of irrigation.'"

Not only then may the organic world, on the Darwinian theory, be conceived as formed in some sense *accidentally*, but we have Mr. Darwin's own words for viewing that formation as "STRICTLY ACCIDENTAL." I say "his words," because I am far from desiring to find Mr. Darwin in anti-teleological fetters. I have carefully given him credit for every theistic expression I noticed, as it was at once my duty and my pleasure to do.

Here I take the opportunity of acknowledging, as I have also done in my second edition, that an American naturalist — Professor Theophilus Parsons, of Harvard University — put forth, more than ten years ago, views* very similar to those I enunciated in my "Genesis of Species," though they were of course unknown to me when I published my first edition. Mr. Wright, however, is mistaken when he states that I am "indebted to Mr. Galton" for my conception of specific genesis, although I made use, with due acknowledgment, of that gentleman's illustration of a conception analogous to mine.

Mr. Wright has been so unfortunate as to misapprehend Mr. Murphy also. Speaking of spheres and crystals, that gentleman is quoted as saying : —

"Attraction, whether gravitative or capillary, produces the spherical form ; the spherical form does not produce attraction."

Upon this Mr. Wright remarks : —

"No abstraction ever produced any other abstraction, much less a concrete thing. The abstract laws of attraction never produced any body, spherical or polyhedral."

But really not only has Mr. Murphy not said *they did*, but his very expression Mr. Wright will, I am sure, regret to see, has been changed by my critic ; and the result is, that Mr. Murphy is unlucky enough to be blamed for what he never said, or apparently thought of saying. This is all the more hard because Mr. Wright goes on to observe, "it was actual forces acting in definite ways that made the sphere or crystal," which is precisely what Mr. Murphy himself said.

Mr. Wright goes on to make a statement which I confess is utterly beyond me. He says : —

"Moreover, in the case of crystals, neither these forces nor the abstract law of their action in producing definite crystals reside in the finished bodies, but in the properties of the surrounding media, portions of whose constituents are changed into crystals, according to these properties and to other conditioning circumstances."

* See the July number of the "American Journal of Science and Art" for 1860.

If this is so, then when a broken crystal completes itself, the determining forces reside exclusively in the media, and not at all in the crystal with its broken surface! The first atoms of a crystal deposited arrange themselves entirely according to the forces of the surrounding media, and their own properties are utterly without influence or effect in the result!

To my mind, I confess, it would appear manifest that those marvelously delicate and complex ice mosses, which at this season occasionally fringe our walls and palings, are not due to forces residing in the atmosphere *only*, but also in the crystalline particles already deposited and in course of deposition.

Professor Tyndal's teaching differs widely from that of Mr. Chauncey Wright. Speaking of the formation of pyramidal crystals of salt, he says:—

"The scientific idea is that the *molecules* act upon *each other*, . . . that they attract each other and repel each other at certain definite points or poles, and in certain definite directions, and that the pyramidal form is the result of this play of attraction and repulsion." *

Mr. Wright seeks to refute the parallelism asserted by Mr. Murphy and by me to exist between crystals and organisms, saying:—

"In organisms, no doubt, and as we may be readily convinced without resort to analogy, there is a great deal that is really innate, or dependent on actions in the organism, which diversities of external conditions modify very little, or affect at least in a very indeterminate manner, so far as observation has yet ascertained."

Here Mr. Murphy and I are fortunately at liberty to invoke in our favor the authority, once more, of Professor Tyndal, who can hardly be deemed even by Mr. Chauncey Wright as incompetent in "experimental philosophy," or as likely to forget "the age of the world in which he lives." In the little work already quoted † he tells us:—

"This tendency on the part of matter to organize itself, to grow into shape, to assume definite forms in obedience to the definite action of force, is, as I have said, all-pervading. It is in the ground on which you tread, in the water you drink, in the air you breathe. Incipient life, as it were, manifests itself throughout the whole of what we call organic nature."

Speaking of a living grain of corn and comparing it with a crystal, he tells us we are bound "to conclude that the molecules of the corn

* Essays on the Use and Limit of the Imagination in Science, 2d edition, 1871. p. 57.

† Ibid., p. 58.

are self-positd by the forces with which they act upon each other. It would be *poor philosophy* to invoke an external agent in the one case and to *reject* it in the other."

Mr. Wright, however, as I have shown, invokes what is innate in the case of organisms and rejects it in the case of crystals, and asserts that in organisms what is innate is so predominant in its action that external conditions "*modify*" them "*very little*."

Passing over how important an admission this is against any effective action of Natural Selection, let us see how it tells against the analogy maintained.

Is not the innate force, as existing in each organism, that which has been educed by antecedent combinations and conditions, just as much and no more external to it than are the forces of the medium to each atom of a crystal? And how does this tell in the least against the analogy which has been asserted, and which really does exist between each chemical unit and each organic unit? Not of course that it is for a moment contended that there is not, as common observation tells us there is, a distinct power and principle, "*vitality*," in the one which is wanting in the other, as well as more or less complexity of organization.

Again we are told, as to organisms, "external conditions are, nevertheless, essential factors in development, as well as in mere increase of growth. No animal or plant is developed, nor do its developments acquire any growth, without very special external conditions." Surely, I hardly needed to be solemnly informed of so very elementary a truth.

Regarding the rules of the "inductive philosophy," Mr. Wright remarks:—

"A stricter observance of these by Mr. Murphy and our author might have saved them from the mistake we have noticed, and from many others,—the 'realism' of ascribing efficacy to an abstraction, making attraction and polarity produce structures and forms, independently of the products and of the concrete matters and forces in them."

In whom, or in what? and what are attraction and polarity if they be not forces? Who ever considered them as acting independently of themselves? Would Mr. Wright prefer that the earth's orbit should be spoken of not as the resultant of gravity and centrifugal force, but as produced by "coming together" and "flying away"? I have, of course, no objection to that mode of expression, but I see no special advantage in it warranting such a departure from usage. It is singular that Mr. Wright himself, on the next page, employs the very "abstractions" he blames others for making use of. He there quotes approvingly the expressions "impenetrability," "mobility," and "im-

pulsive force of bodies," and says "that gravity does really exist and act according to" its laws. It is difficult to see the greater sin in speaking of the "real existence" of polarity than of "gravity." Not only, however, does Mr. Wright quote such expressions, but he uses them himself with the greatest freedom, and without scruple, whenever they suit his purpose. Thus he tells us "that *variability* and *selection* do really exist and *act*," which appear to me quite as much abstractions as polarity or attraction.

Mr. Wright divides "intellectual genius" into three classes: No. 1, "that which pursues successfully the researches for unknown causes by the skilful use of hypothesis and experiment"; No. 2, "that which, avoiding the use of hypotheses and preconceptions altogether, and the delusive influence of names, brings together in clear connections and contrasts in classification the objects of nature in their broadest and realest relations of resemblance"; and No. 3, "that which seeks with success for reasons and authorities in support of cherished convictions."

I might remark on the purely arbitrary character of this classification. But letting this pass, it must be said that class No. 1 is but a poor monster without No. 2; and that No. 1 is frequently, consciously or unconsciously, also No. 3; nor would it be difficult to bring forward an example.

A more real distinction is that to be drawn between the "scientific" and the "philosophical" habits of mind, and under these two great genera come subordinate distinctions of different degrees of importance. Now, a naturalist may attain great scientific eminence without being anything of a philosopher, and, similarly, a philosopher need have little acquaintance with physical science; but from the nature of their respective pursuits a different character of mind tends to be developed. It is from this distinction that we find (as we might *a priori* expect to be the case) such breadth of view, freedom of handling, and flexibility of mind on the part of philosophers who are not naturalists as compared with men great in physical science, who are not at the same time philosophers; a certain rigidity and narrowness seeming to result from the exercise of the mind merely in the arena of physics.

Passing to details of criticism, Mr. Wright proceeds to consider the question of the giraffe's neck, and I am asked a rather startling question: "Can Mr. Mivart suppose that, having fairly called in question the importance of the high-feeding use of the giraffe's neck, he has thereby *destroyed the utility of the neck altogether*, not only to the theory of Natural Selection, but also *to the animal itself*?" At the first glance this looks as if I had brought myself within the grasp of the Society for the Prevention of Cruelty to Animals. But I may, perhaps, be per-

mitted to ask, in return, can Mr. Wright suppose that I ever dreamed that the structures of animals are not useful to them, or that my position is an altogether anti-teleological one? Apparently possessed with some such idea, Mr. Wright proceeds to exhibit the giraffe's neck in the character of a "watch-tower." But this leaves the question just where it was before. Of course I concede most readily and fully that it *is* a most admirable watch-tower, as it also *is* a most admirable high-reaching organ, but this tells us nothing of its *origin*. In both cases the long neck is most useful *when you have got it*; but the question is how it *arose*, and in this species *alone*. And similar and as convincing arguments could be brought against the watch-tower theory of origin as against the high-reaching theory, and not only this, but also against every other theory which could possibly be adduced.

In reply to my objection as to different rate of increase of strength and mass, as the animal increases in size by the supposed transformation, Mr. Wright remarks, that "the neck may have grown at the expense of the hind parts in the ancestors of the giraffe"; and adds, "if we met with a man with a longer neck than usual, we should not expect to find him heavier, or relatively weaker, or requiring more food on that account." I reply, that if we should not do so it would only be from ignorance; for if, *ceteris paribus*, a man's neck was a quarter of an inch longer, he would *necessarily* and *inevitably* be heavier, less strong, and requiring more food, minute though the differences in these respects might be.

In considering criticisms on Mr. Darwin's theory drawn from animal structures, we must not forget how very great an advantage Mr. Darwin has. He has devised a theory according to which any possible utility of any organ is enough to account for its formation. It is amazing, then, that anything whatever should be found for which his theory does not readily account. Much wonder and admiration with regard to that theory has been expressed, because of the way it accounts for so many phenomena, forgetting that this is the necessary consequence of the stand-point he has taken up. Let us suppose, for argument's sake, that the theory is utterly wrong; yet, let but the world be preponderatingly governed by intelligence and beneficence, then the results of that very intelligence and beneficence exhibited in organisms can be made use of to destroy the conception of those qualities in their supreme cause, and to substantiate a theory which, by our supposition, is utterly devoid of truth. It is on this account that Natural Selection can never be completely proved or disproved by physical science in a *posteriori* investigation; for it will be always open to one side to say, the utility not yet shown in any given structure will be shown later, and to the other

side to say, whatever utility you show, though existing in an organ, was not the cause of that organ.

This was no doubt felt by the earlier opponents of Mr. Darwin, who naturally opposed him on *a priori* grounds, and the same feeling has led his supporters to desiderate criticism from the physical-science standpoint, which can never be *quite* conclusive, and can only be approximately so by going into great detail. And this, when done, they in turn affect to sneer at as "*minute*."

Mr. Chauncey Wright's remarks on mimicry do not call for reply, as it is now conceded that imitation occurs where Natural Selection cannot have developed it. In reply to my criticism as to the origin of the mammary gland, my opponent suggests that its development may have been produced by a young mammal's clinging by suction to the body of its dam, this clinging causing sebaceous glands to be hypertrophied, and this hypertrophy causing their secretion to *become nutritious*. I confess this seems to me an extreme supposition.

With regard to sexual selection, Mr. Chauncey Wright asks, "Is it credible Mr. Mivart can suppose that the higher or spiritual emotions, like affection, taste, conscience, ever act *directly* to modify or compete with the more energetic lower impulses, and not rather by forestalling and indirectly regulating them?" I answer, unhesitatingly, "Yes"; and in return say, "Is it credible Mr. Chauncey Wright can suppose they do not?"

As to apes, it is enough to reply, that other animals are also kept in cages, but do not exhibit the phenomena to which I referred.

Passing to the hoods and rattles of poisonous snakes, Mr. Wright asserts that if "their 'warnings' are also used against intended victims, they can only be used either to paralyze them with terror or allure them from curiosity," etc. Has Mr. Wright then never observed the tail of a cat when the animal is watching a mouse?

A somewhat singular exhibition of the use of the imagination occurs where Mr. Wright tells us it may be that "the rattle will serve all the purposes that drums, trumpets, and gongs do in human warfare. The swaying body and vibrating tongue of most snakes, and the expanding neck, and the hood of the cobra, may *serve as banners*." I must submit to be blamed for my "poverty of resources" by one whose "reason" is supplemented by so active an imaginative faculty.

In reviewing my chapter on Independent Similarities of Structure, Mr. Wright replies to my remarks as to characters in placental and implacental mammals which are similar, indeed, but not similar through inheritance:—

"Our author . . . has incautiously left a hostile force in his rear. He has

claimed in the preceding chapter for Natural Selection that it ought to have produced several independent races of long-necked Ungulates, as well as the giraffe ; so that, instead of pursuing his illustrations any further, we may properly demand his surrender."

But such a demand would be futile ; the cases, in fact, being quite dissimilar. With regard to the Ungulates we have the action of similar causes upon organisms which, by the hypothesis, are closely alike ; in the case of the carnivorous and insectivorous beasts we have similar causes acting upon organisms which, by the hypothesis, are fundamentally different.

Certainly, then, if Mr. Darwin's theory is true, we ought to have, in the first case, many similar forms developed ; and we ought *not* to have such in the second case. It is just the difference between adding equals to equals and equals to unequals.

Passing over Mr. Chauncey Wright's exposition* of our Lord's discourse to Nicodemus (in which, I fear, few Darwinians will take any interest), I proceed to notice what Mr. Wright exhibits as "a good illustration" of the origin of species by Natural Selection in the shape of "the growth of a tree." It is so, he tells us :—

"For its branches are selected growths, or few out of many thousands that have begun in buds ; and this rigorous selection has been effected by the accidents that have determined superior relations in surviving growths to their supplies of nutriment in the trunk, and in exposure to light and air. This exposure (as great as is consistent with secure connection with the sources of sap) seems actually to be sought, and the form of the tree to be the result of some foresight in it. But the real seeking process is budding, and the geometrical regularity of the production of buds on twigs has little or nothing to do with the ultimate selected results, the distributions of the branches, which are different for each individual tree."

Now, I willingly accept this illustration, which I propose to turn round and make use of against its author's view, and for the purpose of showing that it exemplifies, not "the origin of species by Natural Selection," but the origin of species by *innate* law, modified by the subordinate action of Natural Selection.

For, in fact, does not every one know that, in spite of these external influences, each kind of tree has a certain general character of growth which is definite and unmistakable. The oak, the fir, the birch, etc., each has its own special *facies*. Mr. Wright does not deny this ; he says :—

* Mr. Wright speaks of "the symbols *water* and the Spirit, which Christians have ever since worshipped." It is certainly difficult to remember the multitude of sects which have appeared since the dawn of Christianity, but the existence of any body of *water-worshippers* strikes me as a novelty.

“The general resemblance of trees of a given kind depends on no formative principle other than physical and physiological properties in the woody tissue, and is related chiefly to the tenacity, flexibility, and vascularity of this tissue, the degrees of which might almost be inferred from the general form of the tree.”

Precisely so. But on what do these physical and physiological properties depend? It is useless to endeavor to avoid the admission; we shall always be compelled by reason to confess the existence, in each seed, of a principle, an *intimus principium* conditioning the evolution of the plant according to its nature and laws. To deny that there is a something giving unity to the composite whole, and unity of a *definite kind*, is to contradict the plain evidence of our senses and our reason.

This internal principle it is which produces the character of each tree's growth, while the special details are determined by the action of external influences upon it. Just in the same way, I believe, that an innate predisposing cause produces the evolution of new species; the special details being determined by subordinate agencies, and amongst them that of Natural Selection. Mr. Wright's illustration suits me so well I will pursue it yet further. He observes:—

“If we could study the past and present forms of life, not only in different continents, which we may compare to different individual trees of the same kind, or better, perhaps, to different main branches from the same trunk and roots, but could also study the past and present forms of life in different planets, then diversities in the general outlines would probably be seen similar to those which distinguish different kinds of trees, as the oak, the elm, and the pine; dependent, as in these trees, on differences in the physical and physiological properties of living matters in the different planets,—supposing the planets, of course, to be capable of sustaining life, like the earth, or, at least, to have been so at some period in the history of the solar system.”

Precisely so once more! In each case forms would be evolved in accordance with that innate potentiality which God has implanted in each case in the matter of which such planet was composed. Not that there, any more than here, all that was potential would become actual, but that the innate potentiality, modified by external influences would be determined in special forms in the production of which the innate power, not the external conditions, would be the main evolving agent.

Mr. Wright seems to consider that the use of such words as “polarity” and “luminosity” tends to discourage the investigation of the laws and conditions by and through which such properties are manifested. Mr. Wright tells us, somewhat dogmatically, that “*definite* vital aggregations and *definite* actions of vital forces exist, for the most part,

in a world by themselves." I should be the last to deny the distinctness of "vitality"; but that certain conditions may determine its sudden and *definite* manifestation, is maintained more strongly than ever by Dr. Bastian, who is industriously pursuing his original inquiries. There is one expression of Mr. Wright's which it will be well to notice; he says: "It is not impossible that vital phenomena themselves include *orders of forces* as distinct as the lowest vital are from chemical phenomena. May not the contrast of merely vital or vegetative phenomena with those of *sensibility* be of such order." I notice with pleasure this hopeful expression. It is most true that there are these differences of order, but there is one more yet. The *intellectual* or rational order is as distinct from the merely sensible as is the sensible from the vegetative, or this last from the chemical. Here we touch the one great and fatal error of so many of our leading naturalists. The confusion of intellect with sensation, of reason with the association of sensible images is, I am persuaded, the fundamental speculative vice of the day. Before concluding this reply there are a few more objections which Mr. Wright does me the honor to make, that must be noticed one after the other.

I am represented as passing an unfair judgment because I say that, though feeling myself incompetent to advance an opinion as to the correctness of Sir William Thompson's astronomical calculations, I yet assert "that the fact that they have not been refuted pleads strongly in their favor, when we consider how much they tell against the theory of Mr. Darwin." For my part I am unable to see how an incompetence for judging astronomical calculations necessarily carries with it an incompetence for judging of the probability of their truth, resulting from their non-refutation by those whose interest would lead them to refute, and who possess the knowledge and ability to enable them ably to handle the requisite questions and calculations.

Again, Mr. Wright does not "see how, with such uncertain, 'fortuitous, occasional, and intermitting' elements" I "could have succeeded in making any calculations at all." I venture to think, however, that an inability to determine the positive time required for the occurrence of certain phenomena in no way involves an inability to fix a minimum period for their development.

Again, in criticising the use of the words "contrivance" and "purpose," Mr. Wright tells us, "the relations of a machine to its uses may be considered in good sound English as contrivances and purposes without thinking of what the inventor *intended*." Now I deny that we can so speak without *implicit* reference of the kind, though we need not make direct or explicit reference. We are also told that

“the proper meaning of the word ‘intention’” is “*concentration*, and the *not* intending of something else.” I should be glad of some reference to authorities as regards this assertion. As a fact the word is used in the sense I have assigned to it. Finally Mr. Wright gives us the application of these new definitions. He affirms that Mr. Darwin is not irrational in asking whether “the Creator intentionally ordered” certain phenomena, because we cannot reasonably make use of the term “intention” in reference to the Creator *at all*.

It is evident, however, that in Mr. Darwin’s opinion we *can* speak of Divine intention in some things, otherwise he would not ask whether we could do so or not even in these. It would be quite superfluous for any one who believed we could do so in *no* case to ask the question with regard to certain special cases. The criticism merely amounts to saying that both Mr. Darwin and I, instead of using the word “intention,” should employ some other, possibly “advertence.” This leaves the substance of my remarks and my criticism of Mr. Darwin quite unimpaired and in full force.

Thus I venture to urge, in opposition to my critic, that far from misinterpreting Mr. Darwin, I have been enabled to bring out more clearly what are his exact position and teaching now, by defining more exactly what was his original theory of the origin of species.

Also, that though by no means necessarily involving irreligious or anti-teleological conceptions, there is no slight danger of the strengthening of these errors by a certain use of the Darwinian theory.

My little book was directed to two objects, — one to show that Natural Selection is not *the* origin of species ; the other, that evolution is perfectly compatible with the strictest Christian orthodoxy : and, in spite of my esteem for Mr. Chauncey Wright, and a careful and respectful consideration of all that he has urged, I cannot at present see my way to retracting or even modifying, in deference to his criticism, even a single passage of my work on “*Specific Genesis*.”

I have the honor to be, sir,

Your most obedient servant,

ST. GEORGE MIVART.

7 NORTH BANK, REGENT’S PARK,
London, December 21, 1871.